We'd like to thank the editor for suggesting an alternative explanation (copied below in black). We have incorporated this by addition of the following to the discussion on page 14: *"Alternatively, given the lack of temporal correlations between CH_4 exchange and soil moisture in these studies, this could also indicate that soil moisture determined at the surface poorly characterised conditions deeper in the profile where greater CH_4 oxidation occurred (Purbopuspito et al., 2006)."*

"Dear Authors,

I have read your revised manuscript and welcome the changes that you made based of the reviews of two annonymous reviewers. There is only one thing that I would like you to consider:

on page 15, you suggest that the lack of relationship between net CH4 flux and water content in Ecuador and Indonesia might be caused by the use of gravimetric water content rather than WFPS.

While I understand the reasoning, I don't think that this is the correct explanation since this correlation was also lacking within any of the sites in Ecuador or Indonesia (where a correlation with gravimetric moisture content should appear, if it exists for WFPS, since the conversion from gravimetric moisture content into WFPS would invoke the same particle density and bulk density). I think the more likely explanation is that the soil depth at which the highest CH4 uptake occured (close to the transition from organic to mineral soil), was not the soil depth that was sampled for soil moisture (top 0.05m), and that this explains the lack of correlation between soil moisture and CH4 uptake.

Thank you for submitting a fine study to Biogeosciences.

Best regards,

Edzo Veldkamp"